I Preliminaries

1.1 Some general observations

Most linguists hold opinions about the ways in which either a language or a grammar may change in the course of time. These opinions are often invoked as the basis for an argument supporting some synchronic analysis. This is sufficiently common for citation to be almost redundant, but consider two recent examples for the sake of illustration. Carden (1976: p77) seeks to choose between two analyses of a given range of data concerning the scope of quantifiers and negatives, although he finds neither analysis very attractive. He considers three dialects and notes that under an analysis using derivational constraints one dialect, which he labels ‘AMB’, can be characterized as the simplest of the three, whereas under the competing analysis, appealing to interpretive semantic rules, the AMB dialect is the most complicated. He then proceeds: ‘if we assume that linguistic change represents a simplification of the grammar, at least at the point at which the initial change takes place’, an argument for the derivational-constraint analysis can be constructed on the basis of two facts: (a) the AMB dialect is the most common, and (b) speakers of the other dialects ‘have been known to change to AMB over a period of months’, while no changes have been

1 The notion of a ‘more simple’ grammar is crucial to Carden’s account and it is not explicated. It seems to be based on the number of rules in the grammars but then he observes in a note that if one looks at a wider range of data it might be the case that under the derivational-constraint analysis the grammar of the AMB dialect has as many (more general) rules as the other two dialects. In any case, until both derivational-constraint and interpretive analyses of scope determination have been worked out in some detail, it will be possible for a partisan of either side to devise a plausible-looking analysis in which the theory he favours gives AMB the simplest grammar of the three dialects’ (p77). Carden’s argument therefore seems to be devoid of any actual content, but here we are concerned with its internal logic, which will turn out to be based on similarly unwarranted assumptions.
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observed in the other direction, from AMB to one of the other two. He then calls attention to the form of the argument, choosing between synchronic analyses but based on ‘a theory of linguistic change motivated by historical data’. Indeed, in his Introduction he notes that ‘an argument combining dialect-variation evidence with an historically-motivated theory of linguistic change will be crucial to the eventual solution’, and he concludes the book with the comment that ‘this sort of argument, if it proves to be widely applicable, offers an approach to the old problem of the synchronic relevance of historical data and the possibility of a genuinely unified linguistic theory’. Despite the importance attached to the argument, no further discussion is offered either of the theory of linguistic change assumed, or the relation between it and the choice between competing synchronic analyses, or the way in which a theory of grammar and a theory of linguistic change may combine to yield a unified linguistic theory. It is crucial to this argument that grammars change only by inexorable simplification and that they cannot become more complicated. From this theory of linguistic change, as Carden describes it, it follows that the grammar of Modern English will be simpler and, presumably, more highly valued than the grammar of any earlier stage of English – a most improbable consequence.

Radford (1976) has quite a different approach to diachrony. In discussing certain complement constructions with causative verbs in Romance, he postulates and compares two grammars, which he labels ‘relational’ and ‘transformational’. One argument offered in favour of the relational grammar is that the transformational counterpart ‘fails to provide any adequate account of the notion of “ongoing linguistic change”’. Over the course of time, there has been a gradual “fusion” of an original two-clause structure into a mono-clause structure and in a relational grammar of Latin and its daughter languages ‘it is possible to discern an ongoing historical process, a unitary “theme” or “direction” of change which makes what has been happening look anything other than arbitrary’. It is by no means clear in what sense the ongoing historical process is discernible in a relational grammar of some synchronic stage of a language, but the notion that the historical process is represented in some more or less transparent way is adduced as support for that general form of grammar. Again, there is no further discussion of the theory of change presupposed, but it is clear that Radford will prefer a particular grammar which represents historical
processes in some form; such discussion as there is is conducted in terms of particular grammars rather than of theories of grammar.¹

Certain aspects of underlying forms postulated by transformational generative grammarians, whether the lexical item which is the input to the phonological rules or the initial phrase marker (or ‘deep structure’) which is the input to the transformational sub-component, sometimes recapitulate their history and take on a form which was actually attested at an earlier stage of the language. Many people have observed that some of the underlying forms postulated by Chomsky & Halle (1968) actually occurred in Middle English. In syntax it has been proposed that in the grammar of Modern English initial phrase markers may contain complementizers with two members, as in I wonder [whether that]COMP Linda left, where that must be deleted subsequently by a transformational rule. This underlying form recapitulates the fact that in Middle English such double-barreled complementizers were attested and a sentence of the form I wonder whether that Linda left was well-formed. In such a situation there is a tendency, to which many linguists have succumbed, to point to the ‘historical validity’ of the underlying forms postulated, as if the fact that they reflect an earlier stage of the language contributes to their plausibility as abstract constituents of a formal, synchronic grammar. It is, of course, not surprising that a grammar should reflect its history to some extent but there is no reason to suppose that ontogeny recapitulates phylogeny as a matter of general

¹ Radford makes even stronger requirements of grammars, that they should reflect ‘the relevance of universalist considerations to language-particular descriptions’. He claims that there is a rule of Subject Raising to higher object position in Latin, cites Postal’s (1974) arguments for ‘a precisely parallel rule’ in English and Japanese, and asserts that ‘there is simply no way of capturing the similarities between these different rules in transformational terms, and thereby attaining a substantively universal characterization of the rule . . . Thus, universalist considerations argue strongly against a transformational characterization of syntactic rules.’ A substantive universal based on three languages is not a basis for a very persuasive argument and, in any case, I have argued that Postal’s arguments for Subject Raising in English have no force (Lightfoot 1976a). Radford assumes that in a relational theory of grammar one can formulate a set of universal rules, i.e. substantive universals. One would like to see one such rule carefully formalized before evaluating the claim (Radford gives the ‘universal’ Subject Raising only what he calls an informal characterization). But if there is such a set of rules, it is not clear that this in itself reduces the class of grammars available to the language learner, which is the basic task of a generative theory of grammar. Unless it is shown that there is such a reduction, the discussion has no bearing on the comparison and evaluation of transformational and relational theories.

For a demonstration that the factual basis of Radford’s claim is also false, see the analysis of Old French by Morin & St Amour (1977).
principle. Therefore, pace Radford, there is no reason for a linguist to view historical recapitulation, when it does occur in an analysis, as lending support or further plausibility.¹

Arguments of the type invoked by Carden and Radford are not uncommon in the linguistic literature. Evidently, diverse opinions are held about the nature of diachronic change in grammars and about the manner in which a synchronic grammar and a theory of grammar should be responsive to data from diachronic change, even if these opinions are often not supported by evidence or even articulated with much care or clarity. Some might argue that there is a causal relationship between the willingness of some linguists to introduce diachronic wild cards into arguments about the nature and form of a synchronic syntactic description and the fact that very little work has been published on the nature of syntactic change and particularly on methodological principles for research in this area. We shall return to an account of the poverty of this domain of enquiry, but in the meantime it is clearly necessary to pose some rather fundamental questions about the investigation of syntactic change and to attempt some straightening up of a few of the basic concepts involved in the enterprise.

1.2 Pre-theoretical reflections

There are several pre-theoretical difficulties for the study of diachronic syntax, such that one must ask whether it is possible to do useful work on historical change and, if so, in what way. If one is to formulate a theory of syntactic change, what will the goals be for such a theory? How can these goals be attained? How will the theory interact with theories of other domains, such as theories of grammar and perceptual strategies? Before providing some answers to these questions, we shall

¹ This has been widely misunderstood. For example, Makkai & Makkai (1976) assume that a transformational grammar of Modern English should provide a model of its history in a very strong sense. They argue that the fact that it fails to do this entails that it should be discarded in favour of a ‘stratificational’ grammar, which, presumably, provides the desired model. They argue that because a transformational grammar is organized in such a way that the phonological rules operate on the output of the syntax, it is in some sense disconfirmed by the alleged fact that certain diachronic syntactic changes in Middle English followed from and were caused by some historically prior phonological changes. If interpreted as a historical model in this way, transformational grammar is apparently consistent with syntactic changes entailing subsequent phonological change, but not with the reverse direction of causation. Needless to say, this is a bizarre interpretation of grammatical models and imposes far too strong a requirement on the predictions which should follow from a synchronic grammar.
1.2 Pre-theoretical reflections

Look first at some of the pre-theoretical difficulties posing problems which by and large do not arise in the study of diachronic phonology. We shall consider five such problems.

(i) A fundamental prerequisite for work in diachronic syntax is that one should be able to compare the grammars of at least two stages of a language. The phonetic and phonological segments of a language are finite and can be listed. But this is not true of sentences, which are potentially infinite, numerically indefinite, and this presents special difficulties for writing a grammar of an early stage of a language. Classical Greek and Early Modern English, for example, are among the most copiously attested of the ancient languages and have extensive corpuses of many different literary genres; also the transitions into Modern Greek and Modern English are well documented at most stages. But even in these cases, there must be severe limitations on the extent to which one can delve into the problems of an ancient language without the aid of a native speaker’s intuition, particularly when one is concerned with the area of the grammar where syntax and semantics interact. Usually one has no knowledge of ungrammatical ‘sentences’, except in the rare instances where a contemporary grammarian may report that certain forms and constructions are not used; one is thus in the position of a child acquiring its first language, who hears sentences being uttered but does not know whether certain other hypothetical sentences are not uttered because they would be grammatically deviant in some principled way or because they have not occurred in his experience simply as a function of chance. One is therefore bound to the texts, which are usually deficient and often lack the crucial examples which will choose one grammatical hypothesis over another. Also, the available texts must be used with caution and with philological skill, since they may represent different dialects or styles. Particular value might be assigned to texts indicating colloquial language, such as letters and dramatic characters like Shakespeare’s Mistress Quickly and Pompey; see Salmon (1965) for a judiciously selected corpus. One must be prepared to allow a grammar to characterize a certain sentence as ill-formed, even though that sentence is actually attested in the surviving documents. The sentence might have been consciously used as an archaism, as an imitation of a foreign construction, or even as deliberately ungrammatical. Thus P. G. Wodehouse puts ill-formed English into the mouth of Monsieur Antoine, the French cook, in Right ho, Jeeves, indicating his non-English origins: ‘Make some
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attention a little. Me, I have hit the hay, but I do not sleep so good, and presently I wake and up I look, and there is one who makes faces against me through the dashed window.’ Similarly, Thorne (n.d.) argues that the language of Leontes in the early part of *The Winter’s Tale* is at times extremely awkward and occasionally downright ungrammatical. He claims convincingly that this is a dramatic device to convey the private delusions and incoherence engendered by Leontes’ jealousy. Critics from Samuel Johnson to Dover Wilson have commented on Shakespeare’s occasional ‘syntactical incoherence’ and ‘ungrammatical style’. In a similar vein, Naro (1976) illustrates the proper use of philological criteria to avoid what might appear to be contrary data.

On the other hand, progress can be made without conjuring up a native speaker of a dead language, if the linguist is prepared to use his own intuitions where obvious generalizations can be made. That is, we reject the traditional restriction that one may use only attested sentences of Classical Greek, Early Modern English, etc., as evidence for one’s hypotheses. Enough is known about Classical Greek for one to make a large number of generalizations, enough to allow most college curricula to include courses on prose composition, where the student composes and the teacher corrects original and otherwise unattested Greek sentences, based on their trained intuitions about the language. We make no apology for doing the same here. To do otherwise is to impose an unrealistic limitation on one’s analyses. Thus, although one is bound to the texts, they must be used with philological skill and one must be ready to characterize certain unattested sentences as well-formed and some attested sentences as ill-formed.

Even with this treatment of the texts, it is probably impossible to write a full grammar of a dead language, albeit as richly attested as Classical Greek and Early Modern English. There will always be crucial gaps in a finite corpus and questions which must be left unresolved. These problems are less acute in diachronic phonology, where one does not need such copious texts in order to do useful work. This was illustrated nicely in the 1971 discovery of a new page of the Gothic Bible, reported and discussed in Szemerényi (1972). Our knowledge of Gothic is based on no more than 280 printed pages, all but ten of which are New Testament translations. The new page added significantly to our knowledge of inflectional morphology and lexical items, but did not permit us to make new inferences about Gothic syntax. King (1969: p141) observes that ‘a corpus of a fixed size will
permit statements about phonology at confidence levels significantly higher than the confidence levels associated with syntactic statements based on the same corpus. We would all feel safer drawing phonological conclusions from the Rosetta stone or the Horn of Gallehus rather than syntactic conclusions.’ One simply needs more data to write a syntactic description. This points to the importance of working with a reasonably well-understood area of grammar and it unfortunately entails severe limitations on the number of languages wherein one can work on diachronic syntax. There are very few languages, apart from Chinese, Tamil, Kannada and those of the Indo-European and Semitic families, which have a rich attestation over a long enough period of time. In order to write a grammar of an early stage of a language, one needs many texts covering many literary genres, and in order to discuss subsequent changes one needs a grammar of a later stage and usually some attestation of a substantial intervening period. The Bantu and Iroquoian languages, for example, do not fulfil this requirement and therefore will not be an appropriate basis for work on syntactic change (reconstruction of the grammars of pre-historical languages is, of course, a very different matter, and something we shall attend to in chapter 3). This limits the number of language-types which can be examined from the viewpoint of syntactic change.

(ii) There are further pre-theoretical difficulties for diachronic syntax beyond those of writing a grammar of a dead language. Students of syntactic change have virtually no legacy from the neogrammarians, and for good reason. The neogrammarians and their contemporaries handed down formidable lists of phonological correspondences related by rules, and discussions of ‘natural’ phonological changes (Vendryes 1902), whereas in syntax the notions of a corresponding form or a diachronic rule made no sense. That is, one can reasonably claim that *chapter, captain are in a way the same word as their historical antecedents, chapter, capitain, and further that these ultimately are derivable (by different routes) from the same (Latin) stem capit-; or even that *brother, pal are (again by different routes – the latter a loan from Romani, derivable from Sanskrit bhrātā) in a sense the ‘same’ as their reconstructed Proto-Indo-European antecedent *bhrater-, the forms being related by historical rules mapping phonemes or allophones into other phonemes or allophones. Basing a theory of phonological change on such correspondences may turn out to be inadequate (as argued by Jakobson, Halle, Kiparsky, Andersen et al.), but it leaves
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scope for much useful work and is not obviously senseless. An analogous view of syntax, however, is incoherent: there is no clear basis for saying that a certain sentence of Old English 'corresponds' to some sentence of Middle English, and there is no reason to claim that a surface structure is mapped by a historical rule into another form occurring in a later stage of the language. Nonetheless there is a good deal of misleading discussion in the more recent literature, which is couched in terms of 'diachronic processes'. Traugott (1969) speaks of a 'diachronic grammar'; Givón (1971), Li (1975a) and Warburton & Prabhu (1975) talk of 'diachronic processes', and seem to visualize some formal device which takes a sentence of an early stage of the language as input and yields a sentence of a later stage. The best one can do is to specify what was a possible sentence at a given stage of a language, and this is the main concern of Delbrück's work.

Of course, there is much more to be said about the neogrammarians view of language. I use 'syntax' differently from the neogrammarians, who used it to cover also what one would now call morphology. Under the rubric of syntax they studied changes in inflections and changes in the use of morphological categories such as subjunctives, infinitives, etc. Jankowsky (1972: p167) points out that 'neither Brugmann nor Paul could conceive of a clear-cut differentiation between morphology and syntax...investigating syntax in its own right simply presupposed a completed morphological analysis'.¹ Moreover, the nineteenth-century view of language was pre-eminently historical; the scientific study of language was in essence a study of its history. Jespersen (1922) began his book with the words: 'The distinctive feature of the science of language as conceived nowadays is its historical character.' So any neogrammarian dealing with syntax would naturally look to history for an explanation of what occurred in their various synchronic descriptions. Historical change in turn was to be explained by the speaker's

¹ In fact, it seems that the neogrammarians did not have a generally agreed definition of the boundaries of syntax. Reviewing Delbrück's Vedic syntax, Whitney (1892: p281) was 'a little surprised to find the formation and value of compound words among the matters discussed at some length...in this work on syntax; the subject is not ordinarily classed as syntactical'. The boundaries were actively disputed. Ries had considerable influence on Brugmann, who abandoned a separate, comparative Indo-European syntax in the second edition of the Grundriss. Ries' (1894) attempt to define the subject argued that syntax should not properly encompass Delbrück's material, only part of which was included in the second volume: Lehr von den Wortformen und ihrem Gebrauch. Lane (1949) claims that it was a result of these disputes 'that, of the actual syntax (by Ries' definition) planned for the Grundriss, only one small volume ever appeared: Die Syntax des einfachen Satzes (1925)'.

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1.2 Pre-theoretical reflections

psychological activity, his ability to see proportional analogies and to create new forms out of old ones by borrowings, blends, etc. At some points one finds such relationships being explicitly drawn in syntax and Paul (1880) sometimes wrote as if he had in mind a notion of synchronic relationships among sentences. For instance, he wrote of the ‘transition’ from verbs to action nouns like transportation, liberation; ‘these may approach the verbal construction; as my transportation from England to Ireland (“I was transported from England to Ireland”)’. There were precedents for this view, as for example in the Port Royal (synchronic) ‘derivation’ of invisible God created the visible universe from three ‘underlying’ propositions which were in the mind. But the neogrammarians never made these ideas as precise as they did many other analytical concepts and it is indisputable that they did not leave behind syntactic discoveries in any way comparable to Verner’s Law or Saussure’s postulation of coloured schwas, or what came to be known as the PIE laryngeals. By and large, despite Paul, they did not use any concept of an abstract formal grammar, with a level of representation distinct from surface structures or with a set of devices to relate one sentence or phrase to another. Also they did not employ recursive rules even for surface structures, and lacked any notion of a ‘generative grammar’, not just of some particular version distinguishing a more abstract level than surface structure. Therefore they were unable to compare the grammars of different stages and to discuss the possible formal relationships between these grammars. Without a theory of syntax along these lines, they could only set up constructional classes and compare the sentences of the various stages, which Delbrück, Brugmann, Löffstedt, Hirt, Wackernagel and others did with characteristic thoroughness and skill. Clearly they understood that it was senseless to write rules mapping a sentence or construction-type of Old English into a ‘corresponding’ Middle English form. Any rate, they did not make the attempt, and the lack of a syntactic legacy in any way comparable to what they left to phonologists can be viewed as a consequence of the theory of language they presupposed.

(iii) A further and related difficulty for diachronic syntacticians is that they cannot begin their work with any useful notion of what constitutes a natural kind of historical change. Given all the phonetic correspondences of the neogrammarians, a phonologist has certain expectations of what kinds of changes to look for in examining a new language. The same types of phonetic changes keep recurring in
language after language and this provides analysts with notions of natural changes. They know that a vocalic segment like a cannot change across the board into a consonantal such as p; that is, there will be no direct historical correspondence mapping aïtar into pitar. Some linguists (e.g. Postal 1968) extrapolate from this and conclude that there can be no synchronic phonological rule changing an a to a p, on the grounds that synchronic rules are subject to a ‘naturalness condition’ that they may not effect an operation which has no correlate from historical change. Some may argue that these intuitive notions usually turn out to be ill-founded, but, given the absence of such historical correspondences in syntax, there can be no basis for parallel notions of natural changes or of natural rules, whatever the merits of such notions.

(iv) Furthermore, given the lack of clear ideas on what constitutes a natural change in syntax, there will be obstacles to reconstructing proto-forms or elements of a grammar of an unattested parent language. Work in historical reconstruction has always presupposed at least an intuitive evaluation metric which would prescribe, for example, that a language with an asymmetric vocalic system or irregular nominal declensions is not highly valued. In reconstructing proto-languages, linguists have presupposed definitions of simplicity of paradigms, naturalness of rules and of changes which must be postulated in order to map the proto-language into one of its daughters, naturalness of certain aspects of grammars, implicational universals. Whatever the validity of these definitions, they have served as essential pre-requisites for phonological reconstruction. There would appear to be no useful analogues for syntax. Most of the candidates for implicational universals, e.g. those of Greenberg (1966), have many unexplained exceptions and virtually nothing is known about the naturalness of syntactic changes or of synchronic rules. We shall return to the viability of syntactic reconstruction in chapter 3, after making some claims about the nature of syntactic change and about a methodology for the study of diachronic syntax. However, on a merely pre-theoretical level, it is clear that the specific techniques used in the reconstruction of the phonological and morphological structure of proto-languages do not hold for similar reconstruction work in syntax.

(v) Many of the difficulties we have discussed stem from the lack of an adequate theory of possible (synchronic) syntactic descriptions. What work has been done by historical syntacticians, although usually thorough and sometimes insightful, has rarely gone beyond obser-